definition in order to facilitate calculations; and by claiming that energy depends merely on positions or velocities, one is able to show that all basic concepts depend only upon immediate experience (p. 18). In Hertz's view this kind of theory was superior to the first in appropriateness and perhaps in correctness, too, but showed weakness in its lack of logical permissibility. For in this view energy is conceived of as a substance, a fact which immediately leads to difficulties when potential energy is considered, either as negative potential energy or as infinite potential energy of a finite quantity of matter. These were some of the difficulties which prompted Hertz to look for a better model (pp. 21–22). His own view is described in the following selection.

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ON THE APPROPRIATENESS, CORRECTNESS, AND PERMISSIBILITY OF SCIENTIFIC THEORIES

A THIRD arrangement of the principles of mechanics is that which will be explained at length in this book. Its principal characteristics will be at once stated, so that it may be criticised in the same way as the other two. It differs from them in this important respect, that it only starts with three independent fundamental conceptions, namely, those of time, space, and mass. The problem which it has to solve is to represent the natural relations between these three, and between these three alone. The difficulties have hitherto been met with in connection with a fourth idea, such as the idea of force or of energy; this, as an independent fundamental conception, is here avoided. G. Kirchhoff has already made the remark in his *Text-book of Mechanics* that three independent conceptions are necessary and sufficient for the development of mechanics. Of course the deficiency in the manifold which thus results in the fundamental conceptions necessarily requires some complement. In our representation we endeavour to fill up the gap which occurs by the use of an hypothesis, which is not stated here for the first time; but it is not usual to introduce it in the very elements of mechanics. The nature of the hypothesis may be explained as follows.

If we try to understand the motions of bodies around us, and to refer them to simple and clear rules, paying attention only to what can be directly observed, our attempt will in general fail. We soon become aware that the totality of things visible and tangible do not form an universe conformable to law, in which the same results always follow from the same conditions. We become convinced that

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the manifold of the actual universe must be greater than the manifold of the universe which is directly revealed to us by our senses. If we wish to obtain an image of the universe which shall be well-rounded, complete, and conformable to law, we have to presuppose, behind the things which we see, other, invisible things—to imagine confederates concealed beyond the limits of our senses. These deep-lying influences we recognised in the first two representations; we imagined them to be entities of a special and peculiar kind, and so, in order to represent them in our image, we created the ideas of force and energy. But another way lies open to us. We may admit that there is a hidden something at work, and yet deny that this something belongs to a special category. We are free to assume that this hidden something is nought else than motion and mass again,—motion and mass which differ from the visible ones not in themselves but in relation to us and to our usual means of perception. Now this mode of conception is just our hypothesis. We assume that it is possible to conjoin with the visible masses of the universe other masses obeying the same laws, and of such a kind that the whole thereby becomes intelligible and conformable to law. We assume this to be possible everywhere and in all cases, and that there are no causes whatever of the phenomena other than those hereby admitted. What we are accustomed to denote as force and as energy now become nothing more than an action of mass and motion, but not necessarily of mass and motion recognisable by our coarse senses. Such explanations of force from processes of motion are usually called dynamical; and we have every reason for saying that physics at the present day regards such explanations with great favour. The forces connected with heat have been traced back with certainty to the concealed motions of tangible masses. Through Maxwell's labours the supposition that electro-magnetic forces are due to the motion of concealed masses has become almost a conviction. Lord Kelvin gives a prominent place to dynamical explanations of force; in his theory of vortex atoms he has endeavoured to present an image of the universe in accordance with this conception. In his investigation of cyclical systems von Helmholtz has treated the most important form of concealed motion fully, and in a manner that admits of general application; through him "concealed mass" and "concealed motion" have become current as technical expressions in German.[1] But if this hypothesis is capable of gradually eliminating the mysterious forces from mechanics, it can

[1] [*Verborgene Masse; verborgene Bewegung.*]

also entirely prevent their entering into mechanics. And if its use for the former purpose is in accordance with present tendencies of physics, the same must hold good of its use for the latter purpose. This is the leading thought from which we start. By following it out we arrive at the third image, the general outlines of which will now be sketched.

We first introduce the three independent fundamental ideas of time, space, and mass as objects of experience; and we specify the concrete sensible experiences by which time, mass, and space are to be determined. With regard to the masses we stipulate that, in addition to the masses recognisable by the senses, concealed masses can by hypothesis be introduced. We next bring together the relations which always obtain between these concrete experiences, and which we have to retain as the essential relations between the fundamental ideas. To begin with, we naturally connect the fundamental ideas in pairs. Relations between space and time alone form the subject of kinematics. There exists no connection between mass and time alone. Experience teaches us that between mass and space there exists a series of important relations. For we find certain purely special connections between the masses of nature: from the very beginning onwards through all time, and therefore independently of time, certain positions and certain changes of position are prescribed and associated as possible for these masses, and all others as impossible. Respecting these connections we can also assert generally that they only apply to the relative position of the masses amongst themselves; and further that they satisfy certain conditions of continuity, which find their mathematical expression in the fact that the connections themselves can always be represented by homogeneous linear equations between the first differentials of the magnitudes by which the positions of the masses are denoted. To investigate in detail the connections of definite material systems is not the business of mechanics, but of experimental physics: the distinguishing characteristics which differentiate the various material systems of nature from each other are, according to our conception, simply and solely the connections of their masses. Up to this point we have only considered the connections of the fundamental ideas in pairs: we now address ourselves to mechanics in the stricter sense, in which all three have to be considered together. We find that their general connection, in accordance with experience, can be epitomised in a single fundamental law, which exhibits a close analogy with the usual law of inertia. In accordance with the mode

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of expression which we shall use, it can be represented by the statement:—Every natural motion of an independent material system consists herein, that the system follows with uniform velocity one of its straightest paths. Of course this statement only becomes intelligible when we have given the necessary explanation of the mathematical mode of expression used; but the sense of the law can also be expressed in the usual language of mechanics. The law condenses into one single statement the usual law of inertia and Gauss's Principle of Least Constraint. It therefore asserts that if the connections of the system could be momentarily destroyed, its masses would become dispersed, moving in straight lines with uniform velocity; but that as this is impossible, they tend as nearly as possible to such a motion. In our image this fundamental law is the first proposition derived from experience in mechanics proper: it is also the last. From it, together with the admitted hypothesis of concealed masses and the normal connections, we can derive all the rest of mechanics by purely deductive reasoning. Around it we group the remaining general principles, according to their relations to it and to each other, as corollaries or as partial statements. We endeavour to show that the contents of mechanics, when arranged in this way, do not become less rich or manifold than its contents when it starts with four fundamental conceptions; at any rate not less rich or manifold than is required for the representation of nature. We soon find it convenient to introduce into our system the idea of force. However, it is not as something independent of us and apart from us that force now makes its appearance, but as a mathematical aid whose properties are entirely in our power. It cannot, therefore, in itself have anything mysterious to us. Thus according to our fundamental law, whenever two bodies belong to the same system, the motion of the one is determined by that of the other. The idea of force now comes in as follows. For assignable reasons we find it convenient to divide the determination of the one motion by the other into two steps. We thus say that the motion of the first body determines a force, and that this force then determines the motion of the second body. In this way force can with equal justice be regarded as being always a cause of motion, and at the same time a consequence of motion. Strictly speaking, it is a middle term conceived only between two motions. According to this conception the general properties of force must clearly follow as a necessary consequence of thought from the fundamental law; and if in possible experiences we see these properties confirmed, we can in no sense feel surprised, unless we are sceptical as to our fundamental

law. Precisely the same is true of the idea of energy and of any other aids that may be introduced.

What has hitherto been stated relates to the physical content of the image, and nothing further need be said with regard to this; but it will be convenient to give here a brief explanation of the special mathematical form in which it will be represented. The physical content is quite independent of the mathematical form, and as the content differs from what is customary, it is perhaps not quite judicious to present it in a form which is itself unusual. But the form as well as the content only differ slightly from such as are familiar; and moreover they are so suited that they mutually assist one another. The essential characteristic of the terminology used consists in this, that instead of always starting from single points, it from the beginning conceives and considers whole systems of points. Every one is familiar with the expressions "position of a system of points," and "motion of a system of points." There is nothing unnatural in continuing this mode of expression, and denoting the aggregate of the positions traversed by a system in motion as its path. Every smallest part of this path is then a path-element. Of two path-elements one can be a part of the other: they then differ in magnitude and only in magnitude. But two path-elements which start from the same position may belong to different paths. In this case neither of the two forms part of the other: they differ in other respects than that of magnitude, and thus we say that they have different directions. It is true that these statements do not suffice to determine without ambiguity the characteristics of "magnitude" and "direction" for the motion of a system. But we can complete our definitions geometrically or analytically so that their consequences shall neither contradict themselves nor the statements we have made; and so that the magnitudes thus defined in the geometry of the system shall exactly correspond to the magnitudes which are denoted by the same names in the geometry of the point,—with which, indeed, they always coincide when the system is reduced to a point. Having determined the characteristics of magnitude and direction, we next call the path of a system straight if all its elements have the same direction, and curved if the direction of the elements changes from position to position. As in the geometry of the point, we measure curvature by the rate of variation of the direction with position. From these definitions we at once get a whole series of relations; and the number of these increases as soon as the freedom of motion of the system under consideration is limited by its connections. Certain classes of

paths which are distinguished among the possible ones by peculiar simple properties then claim special attention. Of these the most important are those paths which at each of their positions have the least possible curvature: these we shall denote as the straightest paths of the system. These are the paths which are referred to in the fundamental law, and which have already been mentioned in stating it. Another important type consists of those paths which form the shortest connection between any two of their positions: these we shall denote as the shortest paths of the system. Under certain conditions the ideas of straightest and shortest paths coincide. The relation is perfectly familiar in connection with the theory of curved surfaces; nevertheless it does not hold good in general and under all circumstances. The compilation and arrangement of all the relations which arise here belong to the geometry of systems of points. The development of this geometry has a peculiar mathematical attraction; but we only pursue it as far as is required for the immediate purpose of applying it to physics. A system of n points presents a $3n$-manifold of motion,—although this may be reduced to any arbitrary number by the connections of the system. Hence there arise many analogies with the geometry of space of many dimensions; and these in part extend so far that the same propositions and notations can apply to both. But we must note that these analogies are only formal, and that, although they occasionally have an unusual appearance, our considerations refer without exception to concrete images of space as perceived by our senses. Hence all our statements represent possible experiences; if necessary, they could be confirmed by direct experiments, viz. by measurements made with models. Thus we need not fear the objection that in building up a science dependent upon experience, we have gone outside the world of experience. On the other hand, we are bound to answer the question how a new, unusual, and comprehensive mode of expression justifies itself; and what advantages we expect from using it. In answering this question we specify as the first advantage that it enables us to render the most general and comprehensive statements with great simplicity and brevity. In fact, propositions relating to whole systems do not require more words or more ideas than are usually employed in referring to a single point. Here the mechanics of a material system no longer appears as the expansion and complication of the mechanics of a single point; the latter, indeed, does not need independent investigation, or it only appears occasionally as a simplification and a special case. If it is urged that this simplicity is only artificial, we reply that in no other

way can simple relations be secured than by artificial and well-considered adaptation of our ideas to the relations which have to be represented. But in this objection there may be involved the imputation that the mode of expression is not only artificial, but far-fetched and unnatural. To this we reply that there may be some justification for regarding the consideration of whole systems as being more natural and obvious than the consideration of single points. For, in reality, the material particle is simply an abstraction, whereas the material system is presented directly to us. All actual experience is obtained directly from systems; and it is only by processes of reasoning that we deduce conclusions as to possible experiences with single points. As a second merit, although not a very important one, we specify the advantage of the form in which our mathematical mode of expression enables us to state the fundamental law. Without this we should have to split it up into Newton's first law and Gauss's principle of least constraint. Both of these together would represent accurately the same facts; but in addition to these facts they would by implication contain something more, and this something more, would be too much. In the first place they suggest the conception, which is foreign to our system of mechanics, that the connections of the material system might be destroyed; whereas we have denoted them as being permanent and indestructible throughout. In the second place we cannot, in using Gauss's principle, avoid suggesting the idea that we are not only stating a fact, but also the cause of this fact. We cannot assert that nature always keeps a certain quantity, which we call constraint, as small as possible, without suggesting that this quantity signifies something which is for nature itself a constraint,—an uncomfortable feeling. We cannot assert that nature acts like a judicious calculator reducing his observations, without suggesting that deliberate intention underlies the action. There is undoubtedly a special charm in such suggestion; and Gauss felt a natural delight in giving prominence to it in his beautiful discovery, which is of fundamental importance in our mechanics. Still, it must be confessed that the charm is that of mystery; we do not really believe that we can solve the enigma of the world by such half-suppressed allusions. Our own fundamental law entirely avoids any such suggestions. It exactly follows the form of the customary law of inertia, and like this it simply states a bare fact without any pretence of establishing it. And as it thereby becomes plain and unvarnished, in the same degree does it become more honest and truthful. Perhaps I am prejudiced in favour of the slight modification

which I have made in Gauss's principle, and see in it advantages which will not be manifest to others. But I feel sure of general assent when I state as the third advantage of our method, that it throws a bright light upon Hamilton's method of treating mechanical problems by the aid of characteristic functions. During the sixty years since its discovery this mode of treatment has been well appreciated and much praised; but it has been regarded and treated more as a new branch of mechanics, and as if its growth and development had to proceed in its own way and independently of the usual mechanics. In our form of the mathematical representation, Hamilton's method, instead of having the character of a side branch, appears as the direct, natural, and, if one may so say, self-evident continuation of the elementary statements in all cases to which it is applicable. Further, our mode of representation gives prominence to this: that Hamilton's mode of treatment is not based, as is usually assumed, on the special physical foundations of mechanics; but that it is fundamentally a purely geometrical method, which can be established and developed quite independently of mechanics, and which has no closer connection with mechanics than any other of the geometrical methods employed in it. It has long since been remarked by mathematicians that Hamilton's method contains purely geometrical truths, and that a peculiar mode of expression, suitable to it, is required in order to express these clearly. But this fact has only come to light in a somewhat perplexing form, namely, in the analogies between ordinary mechanics and the geometry of space of many dimensions, which have been discovered by following out Hamilton's thoughts. Our mode of expression gives a simple and intelligible explanation of these analogies. It allows us to take advantage of them, and at the same time it avoids the unnatural admixture of supra-sensible abstractions with a branch of physics.

We have now sketched the content and form of our third image as far as can be done without trenching upon the contents of the book; far enough to enable us to submit it to criticism in respect of its permissibility, its correctness, and its appropriateness. I think that as far as logical permissibility is concerned it will be found to satisfy the most rigid requirements, and I trust that others will be of the same opinion. This merit of the representation I consider to be of the greatest importance, indeed of unique importance. Whether the image is more appropriate than another; whether it is capable of including all future experience; even whether it only embraces all present

experience, all this I regard almost as nothing compared with the question whether it is in itself conclusive, pure and free from contradiction. For I have not attempted this task because mechanics has shown signs of inappropriateness in its applications, nor because it in any way conflicts with experience, but solely in order to rid myself of the oppressive feeling that to me its elements were not free from things obscure and unintelligible. What I have sought is not the only image of mechanics, nor yet the best image; I have only sought to find an intelligible image and to show by an example that this is possible and what it must look like. We cannot attain to perfection in any direction; and I must confess that, in spite of the pains I have taken with it, the image is not so convincingly clear but that in some points it may be exposed to doubt or may require defence. And yet it seems to me that of objections of a general nature there is only a single one which is so pertinent that it is worth while to anticipate and remove it. It relates to the nature of the rigid connections which we assume to exist between the masses, and which are absolutely indispensable in our system. Many physicists will at first be of opinion that by means of these connections forces are introduced into the elements of mechanics, and are introduced in a way which is secret, and therefore not permissible. For, they will assert, rigid connections are not conceivable without forces; they cannot come into existence except by the action of forces. To this we reply—Your assertion is correct for the mode of thought of ordinary mechanics, but it is not correct independently of this mode of thought; it does not carry conviction to a mind which considers the facts without prejudice and as if for the first time. Suppose we find in any way that the distance between two material particles remains constant at all times and under all circumstances. We can express this fact without making use of any other conceptions than those of space; and the value of the fact stated, as a fact, for the purpose of foreseeing future experience and for all other purposes, will be independent of any explanation of it which we may or may not possess. In no case will the value of the fact be increased, or our understanding of it improved, by putting it in the form—"Between these masses there acts a force which keeps them at a constant distance from one another," or "Between them there acts a force which makes it impossible for their distance to alter from its fixed value." But it will be urged that this latter explanation, although apparently only a ludicrous circumlocution, is nevertheless correct. For all the connections of the actual world are only approximately rigid; and the

appearance of rigidity is only produced by the action of the elastic forces which continually annul the small deviations from the position of rest. To this we reply as follows:—With regard to rigid connections which are only approximately realised, our mechanics will naturally only state as a fact that they are approximately satisfied; and for the purpose of this statement the idea of force is not required. If we wish to proceed to a second approximation and to take into consideration the deviations, and with them the elastic forces, we shall make use of a dynamical explanation for these as for all forces. In seeking the actual rigid connections we shall perhaps have to descend to the world of atoms. But such considerations are out of place here; they do not affect the question whether it is logically permissible to treat of fixed connections as independent of forces and precedent to them. All that I wished to show was that this question must be answered in the affirmative, and this I believe I have done. This being so, we can deduce the properties and behaviour of the forces from the nature of the fixed connections without being guilty of a *petitio principii.* Other objections of a similar kind are possible, but I believe they can be removed in much the same way.

By way of giving expression to my desire to prove the logical purity of the system in all its details, I have thrown the representation into the older synthetic form. For this purpose the form used has the merit of compelling us to specify beforehand, definitely even if monotonously, the logical value which every important statement is intended to have. This makes it impossible to use the convenient reservations and ambiguities into which we are enticed by the wealth of combinations in ordinary speech. But the most important advantage of the form chosen is that it is always based upon what has already been proved, never upon what is to be proved later on: thus we are always sure of the whole chain if we sufficiently test each link as we proceed. In this respect I have endeavoured to carry out fully the obligations imposed by this mode of representation. At the same time it is obvious that the form by itself is no guarantee against error or oversight; and I hope that any chance defects will not be the more harshly criticised on account of the somewhat presumptuous mode of presentation. I trust that any such defects will be capable of improvement and will not affect any important point. Now and again, in order to avoid excessive prolixity, I have consciously abandoned to some extent the rigid strictness which this mode of representation properly requires. Before proceeding to mechanics proper, as depend-

ent upon physical experience, I have naturally discussed those relations which follow simply and necessarily from the definitions adopted and from mathematics; the connection of these latter with experience, if any, is of a different nature from that of the former. Moreover, there is no reason why the reader should not begin with the second book. The matter with which he is already familiar and the clear analogy with the dynamics of a particle will enable him easily to guess the purport of the propositions in the first book. If he admits the appropriateness of the mode of expression used, he can at any time return to the first book to convince himself of its permissibility.

We next turn to the second essential requirement which our image must satisfy. In the first place there is no doubt that the system correctly represents a very large number of natural motions. But this does not go far enough; the system must include all natural motions without exception. I think that this, too, can be asserted of it; at any rate in the sense that no definite phenomena can at present be mentioned which would be inconsistent with the system. We must of course admit that we cannot extend a rigid examination to all phenomena. Hence the system goes a little beyond the results of assured experience; it therefore has the character of a hypothesis which is accepted tentatively and awaits sudden refutation by a single example or gradual confirmation by a large number of examples. There are in especial two places in which we go beyond assured experience: firstly, in our limitation of the possible connections; secondly, in the dynamical explanation of force. What right have we to assert that all natural connections can be expressed by linear differential equations of the first order? With us this assumption is not a matter of secondary importance which we might do without. Our system stands or falls with it; for it raises the question whether our fundamental law is applicable to connections of the most general kind. And yet connections of a more general kind are not only conceivable, but they are permitted in ordinary mechanics without hesitation. There nothing prevents us from investigating the motion of a point where its path is only limited by the supposition that it makes a given angle with a given plane, or that its radius of curvature is always proportional to another given length. These are conditions which are not permissible in our system. But why are we certain that they are debarred by the nature of things? We might reply that these and similar connections cannot be realised by any practical mechanism; and in this respect we might

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appeal to the great authority of Helmholtz's name. But in every example possibilities might be overlooked; and ever so many examples would not suffice to substantiate the general assertion. It seems to me that the reason for our conviction should more properly be stated as follows. All connections of a system which are not embraced within the limits of our mechanics, indicate in one sense or another a discontinuous succession of its possible motions. But as a matter of fact it is an experience of the most general kind that nature exhibits continuity in infinitesimals everywhere and in every sense: an experience which has crystallised into firm conviction in the old proposition—*Natura non facit saltus*. In the text I have therefore laid stress upon this: that the permissible connections are defined solely by their continuity; and that their property of being represented by equations of a definite form is only deduced from this. We cannot attain to actual certainty in this way. For this old proposition is indefinite, and we cannot be sure how far it applies—how far it is the result of actual experience, and how far the result of arbitrary assumption. Thus the most conscientious plan is to admit that our assumption as to the permissible connections is of the nature of a tentatively accepted hypothesis. The same may be said with respect to the dynamical explanation of force. We may indeed prove that certain classes of concealed motions produce forces which, like actions-at-a-distance in nature, can be represented to any desired degree of approximation as differential coefficients of force-functions. It can be shown that the form of these force-functions may be of a very general nature; and in fact we do not deduce any restrictions for them. But on the other hand it remains for us to prove that any and every form of the force-functions can be realised; and hence it remains an open question whether such a mode of explanation may not fail to account for some one of the forms occurring in nature. Here again we can only bide our time so as to see whether our assumption is refuted, or whether it acquires greater and greater probability by the absence of any such refutation. We may regard it as a good omen that many distinguished physicists tend more and more to favour the hypothesis. I may mention Lord Kelvin's theory of vortex-atoms: this presents to us an image of the material universe which is in complete accord with the principles of our mechanics. And yet our mechanics in no wise demands such great simplicity and limitation of assumptions as Lord Kelvin has imposed upon himself. We need not abandon our fundamental propositions if we were to assume that the vortices revolved about rigid or flexible, but inextensible, nuclei;

and instead of assuming simply incompressibility we might subject the all-pervading medium to much more complicated conditions, the most general form of which would be a matter for further investigation. Thus there appears to be no reason why the hypothesis admitted in our mechanics should not suffice to explain the phenomena.

We must, however, make one reservation. In the text we take the natural precaution of expressly limiting the range of our mechanics to inanimate nature; how far its laws extend beyond this we leave as quite an open question. As a matter of fact we cannot assert that the internal processes of life follow the same laws as the motions of inanimate bodies; nor can we assert that they follow different laws. According to appearance and general opinion there seems to be a fundamental difference. And the same feeling which impels us to exclude from the mechanics of the inanimate world as foreign every indication of an intention, of a sensation, of pleasure and pain,—this same feeling makes us unwilling to deprive our image of the animate world of these richer and more varied conceptions. Our fundamental law, although it may suffice for representing the motion of inanimate matter, appears (at any rate that is one's first and natural impression) too simple and narrow to account for even the lowest processes of life. It seems to me that this is not a disadvantage, but rather an advantage of our law. For while it allows us to survey the whole domain of mechanics, it shows us what are the limits of this domain. By giving us only bare facts, without attributing to them any appearance of necessity, it enables us to recognise that everything might be quite different. Perhaps such considerations will be regarded as out of place here. It is not usual to treat of them in the elements of the customary representation of mechanics. But there the complete vagueness of the forces introduced leaves room for free play. There is a tacit stipulation that, if need be, later on a contrast between the forces of animate and inanimate nature may be established. In our representation the outlines of the image are from the first so sharply delineated, that any subsequent perception of such an important division becomes almost impossible. We are therefore bound to refer to this matter at once, or to ignore it altogether.

As to the appropriateness of our third image we need not say much. In respect of distinctness and simplicity, as the contents of the book will show, we may assign to it about the same position as to the second image; and the merits to which we drew attention in the latter

are also present here. But the permissible possibilities are somewhat more extensive than in the second image. For we pointed out that in the latter certain rigid connections were wanting; by our fundamental assumptions these are not excluded. And this extension is in accordance with nature, and is therefore a merit; nor does it prevent us from deducing the general properties of natural forces, in which lay the significance of the second image. The simplicity of this image, as of the second, is very apparent when we consider their physical applications. Here, too, we can confine our consideration to any characteristics of the material system which are accessible to observation. From their past changes we can deduce future ones by applying our fundamental law, without any necessity for knowing the positions of all the separate masses of the system, or for concealing our ignorance by arbitrary, ineffectual, and probably false hypotheses. But as compared with the second image, our third one exhibits simplicity also in adapting its conceptions so closely to nature that the essential relations of nature are represented by simple relations between the ideas. This is seen not only in the fundamental law, but also in its numerous general corollaries which correspond to the so-called principles of mechanics. Of course it must be admitted that this simplicity only obtains when we are dealing with systems which are completely known, and that it disappears as soon as concealed masses come in. But even in these cases the reason of the complication is perfectly obvious. The loss of simplicity is not due to nature, but to our imperfect knowledge of nature. The complications which arise are not simply a possible, but a necessary result of our special assumptions. It must also be admitted that the co-operation of concealed masses, which is the remote and special case from the standpoint of our mechanics, is the commonest case in the problems which occur in daily life and in the arts. Hence it will be well to point out again that we have only spoken of appropriateness in a special sense—in the sense of a mind which endeavours to embrace objectively the whole of our physical knowledge without considering the accidental position of man in nature, and to set forth this knowledge in a simple manner. The appropriateness of which we have spoken has no reference to practical applications or the needs of mankind. In respect of these latter it is scarcely possible that the usual representation of mechanics, which has been devised expressly for them, can ever be replaced by a more appropriate system. Our representation of mechanics bears towards the customary one somewhat the same relation that the systematic grammar of a language bears to a grammar

devised for the purpose of enabling learners to become acquainted as quickly as possible with what they will require in daily life. The requirements of the two are very different, and they must differ widely in their arrangement if each is to be properly adapted to its purpose.

In conclusion, let us glance once more at the three images of mechanics which we have brought forward, and let us try to make a final and conclusive comparison between them. After what we have already said, we may leave the second image out of consideration. We shall put the first and third images on an equality with respect to permissibility, by assuming that the first image has been thrown into a form completely satisfactory from the logical point of view. This we have already assumed to be possible. We shall also put both images on an equality with respect to appropriateness, by assuming that the first image has been rendered complete by suitable additions, and that the advantages of both in different directions are of equal value. We shall then have as our sole criterion the correctness of the images: this is determined by the things themselves and does not depend on our arbitrary choice. And here it is important to observe that only one or the other of the two images can be correct: they cannot both at the same time be correct. For if we try to express as briefly as possible the essential relations of the two representations, we come to this. The first image assumes as the final constant elements in nature the relative accelerations of the masses with reference to each other: from these it incidentally deduces approximate, but only approximate fixed relations between their positions. The third image assumes as the strictly invariable elements of nature fixed relations between the positions: from these it deduces when the phenomena require it approximately, but only approximately, invariable relative accelerations between the masses. Now, if we could perceive natural motions with sufficient accuracy, we should at once know whether in them the relative acceleration, or the relative relations of position or both, are only approximately invariable. We should then know which of our two assumptions is false; or whether both are false; for they cannot both be simultaneously correct. The greater simplicity is on the side of the third image. What at first induces us to decide in favour of the first is the fact that in actions-at-a-distance we can actually exhibit relative accelerations which, up to the limits of our observation, appear to be invariable; whereas all fixed connections between the positions of tangible bodies are soon and easily perceived by our senses to be only approxi-

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mately constant. But the situation changes in favour of the third image as soon as a more refined knowledge shows us that the assumption of invariable distance-forces only yields a first approximation to the truth; a case which has already arisen in the sphere of electric and magnetic forces. And the balance of evidence will be entirely in favour of the third image when a second approximation to the truth can be attained by tracing back the supposed actions-at-a-distance to motions in an all-pervading medium whose smallest parts are subjected to rigid connections; a case which also seems to be nearly realised in the same sphere. This is the field in which the decisive battle between these different fundamental assumptions of mechanics must be fought out. But in order to arrive at such a decision it is first necessary to consider thoroughly the existing possibilities in all directions. To develop them in one special direction is the object of this treatise,—an object which must necessarily be attained even if we are still far from a possible decision, and even if the decision should finally prove unfavourable to the image here developed.

Chapter 12
LUDWIG BOLTZMANN (1844–1906)

INTRODUCTION

Ludwig Boltzmann was born in Vienna on February 20, 1844. He studied at the University of Vienna, receiving his doctorate in physics in 1866. Between 1869 and 1906 he taught in the fields of mathematics and experimental and theoretical physics at Graz, Munich, Leipzig, and Vienna universities. During the seventies Boltzmann devoted himself to the task of explaining the second law of thermodynamics on the basis of the atomic theory of matter. He was able to show that the second law can be understood in principle by combining the laws of classical mechanics with the theory of probability. From this it became clear that the second law of thermodynamics is essentially a statistical law. That is, each thermodynamic system tends toward a state of thermodynamic equilibrium which is by far the most probable state; the entropy function which describes this propensity toward equilibrium and the maximum of which characterizes the state of equilibrium is itself the measure of probability of the macroscopic state. Boltzmann not only laid the foundation of statistical mechanics during these years, but was also able to develop many of its essential elements. His views were further developed by J. W. Gibbs.

In addition to providing this groundwork on statistical mechanics, Boltzmann made significant contributions to the kinetic theory of gases, Maxwell's theory of electromagnetism, and the theory concerning black body radiation. He also wrote many short essays and delivered numerous speeches on different issues; here his studies on topics associated with the philosophy of science occupy quite an important place. In this realm his main interest revolved around the question concerning the very nature of physical theories.

Boltzmann's work in statistical mechanics was strongly attacked by W. Ostwald and other energeticists who rejected real atoms and tried to base all of physical science upon the concept of energy. In

addition Boltzmann's work suffered from misunderstanding on the part of people who substantially agreed with his view, but did not fully understand the statistical character of his reasoning. Although his views were fully justified by discoveries in atomic physics, quite some time was required before the intrinsic value of his investigations was generally accepted. Boltzmann became seriously ill in 1906 and during a period of depression took his own life (September 5, 1906).

The selection chosen here is taken from a lecture series which the scientist gave at Clark University in 1899. The series was entitled *Über die Grundprincipien und Grundgleichungen der Mechanik* ("On the Fundamental Principles and Basic Equations of Mechanics"); it first appeared in *Clark University: 1889–1899, Decennial Celebration,* then was later reprinted in his *Populäre Schriften.* The piece presented here is the first lecture. (Although an English translation of a part of this lecture is available, I have preferred, because of its historical value, to make a new translation of the lecture in its entirety.)

In the lecture series Boltzmann begins by stating that physical theories cannot be developed with a geometrical method such as was used, for example, by Euclid in his *Elements.* However, it is Boltzmann's view that this does not entail the exclusion of all deductive representations of physical theories, but rather that in physics deductive theories must take their starting point from somewhat arbitrary principles which, in principle, are independent of experience. He briefly discusses Hertz's ideas in this regard, showing their strong and weak points. Then he explains that in addition to Hertz's approach other attempts, which remain much closer to classical mechanics, could be made. He next gives an outline of his own view as presented in his book *Vorlesungen über die Principe der Mechanik* ("Lectures on the Principles of Mechanics"), pointing out its strengths and weaknesses.

In the second, third, and fourth lectures, perhaps from a certain viewpoint the most important, Boltzmann showed that in addition to a deductive representation of physical theories an inductive approach is possible and extremely valuable. It was his view, however, that most of the philosophers and scientists who have tried to materialize such an approach did not really succeed in their various enterprises. Boltzmann defended the thesis that a merely inductive approach in the sense of Mill is impossible because in all descriptions of experience, this experience is necessarily transcended. In other words the thesis that science can never transcend experience is wrong; for

this rather simplistic view another thesis is to be substituted along the following lines: science must try not to transcend experience by too much, and never introduce any abstractions which cannot immediately afterward be subjected to verification in experience. Using concrete analyses Boltzmann shows that an inductive approach in physics is valuable and necessary, but that two extremes must be avoided: the view of earlier phenomenologists that a physical theory could be developed without abstractions and idealizations, and the arbitrary and uncontrolled introduction of abstractions which are most often found in textbooks. Boltzmann concludes by observing that many authors combine a deductive and inductive method, a fundamentally unacceptable procedure which necessarily leads to much confusion and many wrong conclusions.

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